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# Complex Kinship Patterns as Evolutionary Constructions, and the Origins of Sociocultural Universals

### Citation for published version:

Barnard, A 2014, 'Complex Kinship Patterns as Evolutionary Constructions, and the Origins of Sociocultural Universals', *Current Anthropology*, vol. 55, no. 6, pp. 766-67. <https://doi.org/10.1086/678972>

### Digital Object Identifier (DOI):

[10.1086/678972](https://doi.org/10.1086/678972)

### Link:

[Link to publication record in Edinburgh Research Explorer](#)

### Document Version:

Publisher's PDF, also known as Version of record

### Published In:

Current Anthropology

### Publisher Rights Statement:

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Chapais nominated seven biologically grounded components, such as pair bonding, from which, he argues, parental control over mating naturally flows. Yet, an important component appears to be lacking from this list, namely parental motivation to control mating. More specifically, even if parents have a capacity to control mate choice, it does not follow that they will actually do so, as motivation is also required. I have argued that the primary motivation of parents comes from having evolved a predisposition to control their children's mating decisions, which in turn has been the outcome of conflicting interests over mating (Apostolou 2010).

In particular, parents and children are genetically related but not genetically identical, which means that their interests in mate choice, to a certain degree, diverge (Trivers 1974). Accordingly, a fitness-maximizing spouse for children is not always a fitness-maximizing in-law for parents, as some traits in a prospective mate give different benefits to the former than to the latter (Apostolou 2007b; Perilloux, Fleischman, and Buss 2011). In consequence, when children exercise mate choice, they make compromises which are not optimal for their parents (Apostolou 2011). This is costly for the latter, and this cost translates into evolutionary pressure exercised on them to place their children's behavior under their control (Apostolou 2014). Parents with this predisposition would be better off than parents without it, because the former would have in-laws who maximize their fitness, while the latter would have in-laws who maximize their children's fitness.

For this predisposition to spread in the population, parents need to be able to control their children's mating behavior, or else those parents with the predisposition would be worse off than those without it, as they would sacrifice resources in attempting to control mate choice without any results. Chapais recognizes that parents have the capacity to influence their children's mating behavior which turns this predisposition beneficial, resulting in a universal parental willingness to control mating. Still, if parents and children had identical interests over mating, children exercising mate choice on their own would choose spouses who would maximize their own, as well as their parents' fitness; thus, there would be no reason for the latter to control mating even if they have the capacity to do so.

The capacity to control mate choice is contingent on the specific cultural context. For instance, in preindustrial societies where individuals of marital age are more dependent on their family for survival and protection, parents have a higher capacity to control mate choice than in postindustrial societies, where children are getting married when they are financially independent from their parents (Apostolou 2010). Accordingly, direct control over mate choice in the form of arranged marriage is found in preindustrial societies and not in postindustrial ones. Yet, in the latter context, the evolved predisposition to control mate choice does not remain silent, but it is expressed differently through manipulation. In particular, in postindustrial societies, parents employ a wide range of manipulation tactics, which include acts such as crying,

giving advice, and lying, in order to influence indirectly their daughters' and sons' mating decisions (Apostolou 2013).

One insight that Chapais offers in his paper is that although certain social patterns, such as arranged marriages, are not found in all human cultures, they are universal in the sense that they are consistently present in similar sets of social circumstances—they are therefore context-dependent universals. In the mate choice domain, the parental disposition to influence mating is not context-dependent, but the way it is manifested is. Thus, in a context where parents have a capacity to control mate choice directly, arranged marriage will emerge, and in contexts where their capacity is limited, manipulation over mating behavior will emerge.

Overall, the notion that many traits constitute biological grounded categories operating as templates for the creation of cultural variants in form and meaning constitutes a fruitful way for studying the interactions between evolved predispositions and environmental contexts in giving rise to observed cultural and behavioral patterns.

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I congratulate Bernard Chapais on his attempt to explain kinship in terms of sociocultural universals, although I have some slight misgivings about what is missing from his analysis, namely a consideration of language. For many years as a kinship specialist I have long been intrigued by the degree of similarity, as well as difference, in kinship structures. As a recent convert to the interplay between social and biological aspects of a larger anthropology (to which Chapais has greatly contributed), I think he is definitely on the right track.

Within social or cultural anthropology, there have been few attempts to raise such questions. Malinowski (1944:75–84) was one exception, although his attempt was weak and simplistic. Starting from a foundation in primate studies or biological anthropology more generally does offer more hope, as indeed Piddington (1957:48–51) suggested. In a way, I see Chapais's notion of building cultural diversity on a biological foundation in this vein. However, the fundamental difference between humans and *Pan* is that the former categorize and the latter do not. Therefore, there cannot be an evolution from chimpanzee tendencies toward incest avoidance (see also Fox 1975) to human categories of avoidance. The two are in fact quite different. In human kinship the categorization is absolute, which is, after all, how incest is defined. That is also how the incest can be both "natural" (in existence in all human societies) and "cultural" (defined differently in each human society), as Lévi-Strauss (1969:12–25) noted. In this article, Chapais pays only minimal attention to language, which for me has to be the main defining difference between

human and chimpanzee cultures. Among humans language is universal; among chimpanzees it is nonexistent. Primate communication is something very different.

The long-term evolution of kinship (at least since *Homo ergaster*) should take account of the emergence of language, or at least the interplay between the development of linguistic categories and the classification of relatives (Barnard 2008). In the shorter term (since the beginnings of language proper), the use of kinship relations and the categories that represent them is much more nuanced (see Barnard 2013). Parts of what Chapais argues imply this, but he does not directly confront the problem. For example, class 6 (composite-exclusive) traits include recognition of patrilineal relatedness and affinal ties, and class 7 (cognitively-generated) traits include traits that require group recognition and language. Fortes (1983) wrote definitively on the importance of jural rules (which require articulation through language) and the fact that these are what differentiates humans from other primates. Chapais (2008:74–76) indeed cites and quotes from Fortes's work on this in *Primeval Kinship*, though he touches on it only peripherally in the present article. It is a great pity that Chapais does not pay more attention to the importance of language in kinship (e.g., in the evolution of kin classification), for therein seems to lie a level beyond composite traits that has yet to be explained.

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Bernard Chapais's attempt to create a proper synthesis of biological and cultural approaches to human marriage, social systems, and kinship is both much to be welcomed and long overdue. I approve thoroughly and agree with most of what he says. I will raise only two issues, neither of which should be considered fatal flaws in his argument, but which are, I think, an important part of the story nonetheless and we should have them correct.

The first revolves around the human mating system. Chapais seems to take it for granted that humans are monogamous, and that pair-bonds exist to enable biparental care. This is odd for two reasons. One is that it would seem to confuse monogamy (as a marriage system) with pair-bonding (a social, often temporary, arrangement between couples). Humans undoubtedly pair-bond; they are much less often monogamous in the sense that we can use this term of primates. The other point is that it may well be true that human males (occasionally) engage in parental care—though their efforts can hardly be said to be impressive—but this does not mean to say that biparental care is the primary function of pair-bonding in humans.

There have been just three plausible hypotheses offered for

the evolution of monogamy in primates (biparental care, dispersed females, and infanticide). A proper analysis of the comparative data demonstrates—rather uncompromisingly—that the first two only appear after the adoption of monogamy as a mating system; high infanticide risk, on the other hand, always precedes the adoption of monogamy (Dunbar 1995; Opie et al. 2013). Convention in science, as in everyday life, holds that causes precede their consequences, not follow them. (It would not be sensible to suggest, for example, that species adopt monogamy in order later to permit paternal care: that would require a species to evolve an adaptation before it has any benefit from it.) Sadly, recent attempts to examine this question (notably those cited by Chapais, such as Gavrillets 2012 and Lukas and Clutton-Brock 2013) have not always been as clear-thinking in their analyses. This is particularly obvious in respect of the claim that monogamy in primates evolved because females started to live in dispersed ranges that were too big for males to defend. In fact, not only do monogamous females not have per capita ranges that are larger (or smaller) than those of polygamous territorial species, but almost all males from monogamous species have the capacity to defend territories large enough to include the ranges of five or more females: this was shown 30 years ago (Dunbar 1988), and new analyses confirm and extend these findings. In short, the core assumption of the Lukas/Clutton-Brock claim does not hold. Female dispersion certainly explains monogamy in many mammals, but it does not do so in primates. Nor does biparental care. I am impressed by the fact that Chapais does not even mention infanticide as a possibility. The idea of infanticide clearly causes so much angst that, evidence and logic notwithstanding, its history has been dogged by desperate and largely specious attempts to duck it (or claim it doesn't exist) ever since it was first proposed by Sarah Hrdy 40 years ago. That's a pity: it might make sense of a lot of things both primates and humans do, one of which is the fact that both mate-choice processes and pair-bond intensity tend to be female biased (see, e.g., Palchykov et al. 2012).

The second issue I want to raise concerns multilevel social systems. Chapais argues that in primates, "the social group is the highest level of social organization: groups do not combine to form higher level social entities." This isn't strictly true. With the possible exception of the monogamous species, all primates live in multilevel social systems that are highly substructured (Hill et al. 2008; Kudo and Dunbar 2001). In some cases, these grouping levels may be quite dispersed; in other cases, they may even involve polyspecific associations with allied species (e.g., African forest guenons; Korstjens, Lehmann, and Dunbar 2014, in preparation). The capacity to manage multilevel sociality is one of the hallmarks of being a primate (or, at least, a certain kind of cognitively advanced primate); what is true is that humans have been able to use this capacity to create additional levels of grouping above the more conventional ones. What is, perhaps, interesting is that in no culture does human kinship terminology extend out